tives were obtained on 81 days, showing sun spots on 18, distributed as follows: Two in September, two in December, five in January, five in February, three in March and one in April. Visual observations of the sun in May indicate the same low state of solar activity.

STANDARD CLOCK.

Preparations are now in progress for installing a standard clock in a hermetically sealed case to be kept in a double-wall chamber at a constant temperature. The device for keeping the temperature constant is entirely similar to that now employed so successfully in the temperature room for testing chronometers. I consider this subject as one of the most important for the future of the fundamental work of the Observatory, and no pains or expense ought to be spared in securing the best possible performance of a standard clock under the conditions above described. I regret that the preparations for the eclipse so completely occupied the resources of the Observatory that this important matter has had to be laid aside until the present time.

*AIMS AND METHODS OF STUDY IN NATURAL HISTORY.**

I INVITE your attention to an old but still fruitful topic, namely, the aims and methods of study in natural history. It is a well-worn theme, but one that will retain its interest to the naturalist so long as natural history remains a progressive subject; and I venture to think that it was never more timely than at the present period of intense activity in natural science, of rapid development of new aims and methods, and of continually shifting point of view. How great the changes have been in the last twenty or even ten years is, I dare say, hardly realized by many of the younger generation of naturalists to-day. To appreciate their full extent one must be old enough to have passed his student days in the sixties and seventies, at a time when it was still possible to discuss the truth or error of the evolution theory; when the germ theory of disease was itself no more than a germ; when a gastrula or a karyokinetic figure was a thing to be spoken of with bated breath, but not to be looked upon when there were no oil-immersion-lenses or Abbe illuminators, no automatic microtomes, no ribbon-sections, no chromosomes or centrosomes, no shaking of eggs, no 'taxes' or 'tropisms'; when to adopt the career of a naturalist was to face the imminent prospect of extinction in the struggle with the environment, and to incur the half-admiring, half-contemptuous compassion of one's relatives and friends.

Speaking as I am in the presence of some of those who guided my own first tottering footsteps along the pathway of science, I feel some hesitancy in claiming a place among those veterans of the old guard; but I am nevertheless able to recall days when we had to do without all the things I have mentioned, as well as a good many others, both material and spiritual, that are now considered the very bread of life in the day's work. I will confess, too, that I am old enough to be at times lost in wonder at the child-like serenity with which the modern student will accept many of these matters, which cost such travail of the spirit, and at the distant epoch to which I have referred would have produced a sensation throughout the scientific world. When, for instance, Kleinenberg made the famous declaration 'Es gibt gar kein mittleres Keimblatt' it seemed to us that the sky must fall on such a blasphemy. We have changed all that. Cite those memorable words to-day, at the climax of your cautious discussion of the

* Presidential Address delivered at the annual dinner of the American Society of Naturalists, Baltimore, December 28, 1900.

germ-layer theory, and your *fin de siècle* student merely remarks stolidly, as he reels off a yard or two of ribbon-sections from his Minot microtome, "Of course not; but what is the use of talking about such an antediluvian myth?" It is enough to make Balfour turn in his grave!

I do not propose to review the advance of discovery in recent years, but only to offer a few reflections on the progress of our aims, methods and standpoints, taking as my point of departure Louis Agassiz's delightful little book, entitled 'Methods of Study in Natural History,' published in 1863. In this work we find a clear and simple exposition of the aims and methods of natural history as they appeared to a great naturalist and teacher before the theory of evolution had wrought its wonderful transformation in natural science. We all know that, as far as that theory was concerned, Agassiz ranged himself on the side of a losing cause, believing, to quote his own words, that naturalists were chasing a phantom in their search after some material gradation among created beings such as that theory demanded, though he was constrained to the admission that 'this notion' had a certain fascination for the human mind. I am here concerned with Agassiz's position on this question only in its bearing on his aim and method. It was Agassiz's aim, first, to observe phenomena with all possible accuracy; and, second, to arrange and classify them in order to discover the 'natural affinities' of living things. His method, on the all-importance of which he was never weary of dwelling, was that of his master, Cuvier, *comparison*. "The true method of obtaining independent knowledge" he says "is this very method of Cuvier's—comparison." "The education of a naturalist now consists chiefly in learning how to compare." It was not Agassiz's aim to analyze and explain phenomena, as Darwin was attempting to do. His whole theory of organic creation precluded such an aim; for existing phenomena of life were viewed as the result, not of progressively operating causes, but of special creation, and 'natural affinities' among living things were but the expression of creative thought. It was enough for him to observe, compare and classify. In his work one is everywhere struck with the eager and enthusiastic delight that he took in the facts of natural history for their own sake. The key note of Agassiz's work was, in short, *the love of nature*, and his remarkable success as a teacher was mainly due to his power of inspiring a like enthusiasm in others. Such, in few words, were what seem to me the characteristic features in Agassiz's aim and methods. They may have for us later naturalists a useful lesson, both in their agreement with, and their contrast to, some of the latest *dicta* of modern writers on scientific method.

Leaving aside for the moment the subject of experimental physiology, we may say broadly that the progress of natural history since Agassiz's time has been along three general lines of study, though no very definite line of demarkation between them can be drawn. First came the development of comparative morphology, dominated by Agassiz's method of observation and comparison, but largely inspired by a theory of organic forms that was the very antipode of his own. Here belong the elaborate and exact modern investigations on general and systematic zoology and botany, on geographical and geological distribution and on comparative anatomy and embryology. In all these, a leading motive was to search for natural affinities and to interpret them in accordance with the theory of evolution. It has been a laborious and persistent quest, carried forward on a vast scale; and there is now hardly a corner of the plant or animal kingdom into which it has not been pressed. Its point of departure was primarily given by the comparative anatomy

of existing forms of life, supplemented by that of extinct forms. Almost from the start, however, it was evident that the data derived from those sources were sufficient without the additional evidence afforded by the facts of embryological development. Despite the high degree of validity possessed by the paleontological evidence, the record is, and is likely always to remain, too meager to guide us to the broader results we seek. Without the aid of embryology, comparative anatomy, with all its wealth of data, gives us hardly a hint of some of the most fundamental relations of living things. The high value of the embryological evidence was therefore early recognized ; and with the progress of research it played a more and more important rôle in the examination of genealogical problems.

It seems a singular irony of fate that Agassiz, an anti-evolutionist, should have singled out as the most important result of his life-work a discovery in embryology, which, in connection with the generalizations of von Baer and Darwin, was destined to form one of the watchwords of a coming generation of evolutionists. "I have devoted my life to the study of Nature, and yet a single sentence may express all that I have done. I have shown that there is a correspondence between the succession of forms in geological times and the different stages of their growth in the egg—this is all." In another place he urges young students to turn to the study of embryology ; for here, he says, lies 'an inexhaustible mine of valuable information—where we shall find the true facts by which to determine the various kinds and different degrees of affinity which animals bear not only to one another, but also to those that have preceded them in past geological times.' How little he foresaw the use which embryologists were soon to make of this principle or the lengths to which they would go in its application. It was in that very year that Fritz Müller published the famous little book entitled 'Facts for Darwin,' which contained the first clear outline of the recapitulation theory and marked the beginning of the embryological search for genealogies, continued with so much ardor by Haeckel, Semper, Claus, Dohrn, Balfour and a hundred others. Many of us have eagerly followed the phases of that long quest or have sought to make our own modest contributions to it. We know how many puzzling problems of comparative morphology it has brought to a solution, how great an impulse was given to the investigation of natural affinities by the formulation of the recapitulation theory by Müller, Haeckel and their followers. I would be the last to question the immense interest and value of the results that have thus been achieved in the field of genealogical inquiry. And yet I believe that when these results, together with those derived from all other sources, are broadly viewed, we are constrained to the admission that comparative morphology as a whole has thus far solved only minor problems of descent, and that naturalists as a body are beginning to turn their attention in other directions. Let any one who doubts this compare the present attitude of naturalists towards some of the more general problems of descent with that of fifteen or twenty years ago. At that time the burning questions of zoological morphology centered in far-reaching genealogical hypotheses such as the *Gastræa* theory, the *Trochophore* theory, the *Nauplius* theory, the origin of vertebrates, the origin of metamerism, or the derivation of bilateral animals from medusoid or polypoid forms. They still remain questions of very high interest, but they are no longer the leading questions of the day ; and we may as well admit the truth that interest in them is beginning to wane, temporarily perhaps, but unmistakably.

It will be worth our while to inquire into the reasons for this.

First, we cannot repress a certain feeling of dissatisfaction at the vagueness of our conclusions regarding many of these major problems. Our knowledge of the anatomy and development of the leading types of life is still very far from complete—indeed, the field before us remains so vast that we may never hope to exhaust its possibilities of research. We have, nevertheless, gained a fairly clear view of the general outlines of the system. But have we reached substantial agreement regarding the natural affinities of the great types? In a few cases, yes; but I think the candid naturalist must also reply, in most cases, no. How is it with that time-honored problem, the origin of vertebrates, in one way the most interesting of all, involving as it does our own remote ancestry? How is it with the origin of annelids or mollusks, of echinoderms, of platodes, of round worms or molluscoids? What are the historical relationships of the higher types to the Cœlenterata, of bilateral to radial forms, or of Metazoa to Protozoa? I dare say most of the morphologists present hold more or less definite views on these questions—if I, for one, am charged with holding such views on the zoological side I shall not defend myself or deny that all these are questions of high interest to me. But have we reached definite conclusions on which we are substantially agreed? I fear that a general discussion of the zoological members of this society would elicit but too emphatic a negative reply, and that a similar symposium of our botanical brethren would not set us a better example of unanimity. I do not doubt that the progress of research will in time bring us much nearer to a definite solution of these great problems; though it lies in the nature of the case, that we can never attain complete certainty. In the mean time, we may as well admit that in the application of the embryological evidence to the broader problems of descent the recapitulation theory has encountered so many difficulties, undergone so many modifications and limitations, that investigators have in a measure wearied of their wanderings through the scholastic mazes of ancestral and secondary characters, of palingenesis and cenogenesis, of primary and adaptive forms and the like, and have sought for new interests and fresh motives of study. This is clearly apparent in the changed character of the more recent papers in embryology, which devote far less attention than those of ten or fifteen years ago to 'genealogische Betrachtungen' that once formed their inevitable climax. The relative decline of interest in genealogical questions is partly due, I think, to a healthy reaction against the inflated speculation into which morphologists have too often allowed themselves to fall; but it is also in large measure a result of the growing feeling that the solution of the broader problems of genealogy still lies so far beyond our reach that we would better turn for a time to the study of questions that lie nearer at hand and are, to say the least, of equal interest and importance.

We here arrive at a consideration of the two other great lines of progress to which I have referred. The first of these includes the modern developments of the cell theory, which have perhaps contributed equally with the evolution theory to the unification of biological knowledge. I need not dwell on the fundamental importance or the fascinating interest of the general results that have been attained in this field. The point on which I would lay emphasis is that investigation in this direction has only in very minor degree been inspired by the evolution theory or influenced by the historical point of view. The study of the cell, whether morphological or physiological, has been inspired by the desire to penetrate more deeply into the mechanism of

the existing living body. It has established a fundamental unity in the organization and modes of activity of living things, but it has thus far taught us little or nothing regarding their origin and progressive transformations. The interest of the results of cell-research, therefore, is of a different kind from that attaching to the genealogical problems of comparative morphology, and the one has grown, in some measure, at the expense of the other.

A no less potent influence has been the rapid infusion of experimental methods into morphological research, which forms the third line of progress in question, and is fast becoming the characteristic feature of latter-day biology; and with this we may briefly regard the far older subject of experimental physiology. When we regard the novelty and importance of the results already attained through these methods, it seems strange that morphologists were so long content to leave them to the almost undisputed monopoly of the physiologists; and I think that zoologists must admit further that, until recently, they have lagged behind the botanists in this regard. It would, however, be wide of the mark to maintain that experimental methods in morphology are a new product of the day. Did not Bacon, in the 'Novum Organum,' urge that living things are especially adapted for experiment, and in the 'Nova Atlantis' even project a scientific institution for experimental researches with reference to the problem of variation?* More than a century before our time Trembley, Bonnet and Spallanzani showed how rich a field lay in the experimental study of regeneration; and Darwin later taught us what a wealth of suggestive results could be drawn from the long-continued experiments of breeders of domestic plants and animals. Nevertheless, it is only very recently that a definite program of experimental morphology has been laid out, and that naturalists have begun to address themselves seriously to the task.

* Osborn, 'Greeks to Darwin,' pp, 92, 93.

The revival of experimental methods in morphology is only in part due to a reaction against genealogical speculation. It is in at least equal measure due—and here we touch on a point that is vital to my present purpose—to the closer relations that have sprung up between morphology and physiology, and to the *development of comparative methods on the part of physiologists.* Animal physiology, long confined almost exclusively to the study of vertebrates, at last broke away from its earlier traditions and entered upon a new career, in the course of which it amalgamated with morphology. The traditional line between morphology and physiology thus faded away in zoology, as it had earlier done in botany, as naturalists advanced from either side into a neutral zone of inquiry devoted to the physiology of the lower animals and of the cell, to the activities of one-celled organisms, and to experimental studies on regeneration and development, and on cell-morphology; while in the study of habit, instinct, variation and inheritance the psychologist and even the sociologist have made common cause with us. We may well congratulate ourselves on such a solidification of aim and on the accompanying increase in the exactness and order of our method, and this not merely because of the value of the results attained, but in no less degree through the revival of interest in natural history, in the older sense of the word, that has accompanied it. We see the signs of this revival in many directions—in precise and far-reaching inquiries into the habits and instincts of insects and birds, and the life of animal communities; in renewed and more accurate ecological studies on plants and animals of almost every group, in the increasing interest in systematic zoology and botany, in the extended examination of the plankton of inland waters and the sea, in the rapid

development of exact statistical methods in the study of variation, and in many other ways, among which we should not forget the mention of the development of courses of instruction in the so-called 'Nature-study,' and the recent appearance of admirable text-books in which anatomical detail is largely—perhaps too largely—subordinated to the older natural history. I think, too, that we have a right in this connection to point to the influence that such associations as this Society have exerted in widening the range of common interests and fostering the spirit of scientific fellowship and cooperation.

With these changes has come a better understanding between the field naturalist and the laboratory morphologist and physiologist, who in earlier days did not always live on the best of terms. I shall never forget the impression made on me many years ago, shortly after returning from a year of study in European laboratories, by a remark made to me in the friendliest spirit by a much older naturalist, who was one of the foremost systematic and field naturalists of his day, and enjoyed a world-wide reputation. "I fear," he said, "that you have been spoiled as a naturalist by this biological craze that seems to be running riot among the younger men. I do not approve of it all." I was hardly in a position to deny the allegation; but candor compels me to own to having had a suspicion that while there may have been a mote in the biological eye, a microscope of sufficient power might possibly have revealed something very like a beam in that of the systematists of the time. However that may have been, it is undeniable that at that period, or a little later, a lack of mutual understanding existed between the field naturalist and the laboratory workers which found expression in a somewhat picturesque exchange of compliments, the former receiving the flattering appellation of the 'Bug-hunters' the latter the ignominious title of the 'Section-cutters,' which on some irreverent lips was even degraded to that of the 'Worm-slicers'! (For the sake of completeness it may be well to add that at a later period the experimental morphologists fared no better, being compelled to go through the world under the stigma of the epithet 'Egg-shakers.') I dare say there was on both sides some justification for these delicate innuendoes. Let us for the sake of argument admit that the section-cutter was not always sure whether he was cutting an *Ornithorhynchus* or a pearly *Nautilus*, and that at times perhaps he did lose sight of out-of-doors natural history and the living organism as he wandered among what Michael Foster called the 'pitfalls of carmine and Canada balsam'; but let us in justice mildly suggest that the bug-hunter, too, like Huxley's celebrated old lady, was sometimes a trifle hazy as to whether the cerebellum was inside or outside the skull, and did not sufficiently examine that hoary problem as to whether the hen came from the egg or the egg from the hen, and by what kind of process. The lapse of time has in truth shown that each had something to learn from the other. The field naturalist came to realize that he could not attain right conclusions in the investigation of the larger problems before him without more thorough studies in anatomy and development. The laboratory morphologist learned better to appreciate the fact that his refined methods of technique are after all but a means toward the better understanding of the living organism and its relation to its environment. On both sides, accordingly, the range of common interests and sympathies was extended; and some of the splendid monographs of recent years bear witness to the value of the results that have flowed from the combination of anatomical, embryological, systematic and ecological research.

But now, in the last place, we encounter in the recent writings of some of the experimental morphologists a singular attitude of mind toward other methods of study and, in particular, toward the comparative method and the historical point of view in biology generally, for it is seriously maintained that the scientific study of organic nature is possible through experiment and through *experiment alone.* That I am not overstating the case will be evident from the following citations from recent utterances by an eminent leader in this field. "The comparative method in morphology," he says, "is in itself not science, but only a preparation for scientific work." Speaking for the self-styled 'rational morphologists' he says, "We have not *a* method of scientific morphology, but *the scientific morphological method.* There is but one productive method, and that is our method." The historical point of view in comparative morphology is of wholly minor value. Even could we accurately determine the ancestral origin of plants and animals—which in point of fact we cannot do—we should still not have solved the real problem—namely, the laws in accordance with which evolution has taken place. The most complete acquaintance with phylogeny would give us only an ancestral portrait-gallery, nothing more than a 'photograph of the problem.' Only through systematic experiment can we unveil the nature and limits of the power of transformation that lies at the root of the evolutionary process.

We may as well admit forthwith that there is a large element of truth in this bold claim, and it is well to recall how prominent a place the experimental evidence of evolution held in Darwin's mind. The history of science shows incontestably that only through experiment, through deliberately calculated and precise alterations in the conditions under which phenomena occur, can we attain the limits of scientific analysis. So long, therefore, as the naturalist limits himself to the study of vital phenomena under natural conditions, he falls short of the highest ideal of scientific investigation. For my part, I am wholly ready to admit that the introduction of experimental methods into morphology is the most momentous step in biological method that has been taken since the introduction of such methods into physiology by Harvey and Haller. As regards the comparative method, I do not overlook the force of the argument that when comprehensive conclusions are attained by the mere elimination of facts that are not common to all the individual cases compared, those conclusions must have a more limited content than the collective data on which they are based, though I suspect that it would not require a very long search to discover a fallacy lurking here. But seriously to maintain that the non-experimental comparative study of nature is not science is an efflorescence of enthusiasm at which one could hardly repress a smile did it not involve so serious a blunder.

Now I certainly shall not undertake such a work of supererogation as a defense of the comparative method in natural history. Moreover, the statements cited were, I believe, intended mainly as a protest against too free genealogical speculation, and perhaps conveyed more than their author really intended. Yet the undoubted truth that they embody is masked by a form of expression so misleading, that even before this audience of naturalists I shall venture to place beside them the words of one or two of those best qualified to pass judgment on scientific method in the domain of physical science, which may rightly claim to be the experimental science *par excellence.* Helmholtz, in a discourse on the relation of natural science to general science, delivered at Heidelberg forty

years ago, said: "It is not enough to be acquainted with the facts; scientific knowledge begins only when their laws and causes are unveiled. Our materials must be worked up by a logical process; and the first step is to connect like with like and to elaborate a general conception embracing them all. Such a conception, as the name implies, takes a number of single facts together, and stands as their representative in our mind. We call it a general conception, or the conception of a genus, when it embraces a number of existing objects; we call it a law when it embraces a series of incidents or occurrences." What is the first example by which Helmholtz illustrated his meaning? It is one drawn not from experimental science, but from comparative anatomy, namely, the correlations of structure shown by the Mammalia. What was Helmholtz's estimate of the historical point of view in biology? Here are his own words, spoken ten years after the appearance of the 'Origin of Species': "The facts of paleontological and embryological evolution were enigmatical wonders as long as each species was regarded as the result of an independent act of creation, and cast a scarcely favorable light on the strange tentative method which was ascribed to the Creator. Darwin has raised all these isolated questions from the condition of a heap of enigmatical wonders to a great consistent system of development, and established definite ideas in the place of such a fanciful hypothesis as, among the first, had occurred to Goethe, respecting the facts of the comparative anatomy and the morphology of plants."

But, the 'rational morphologist' may reply, these words were spoken thirty years ago, and conditions have profoundly changed since Helmholtz passed this too favorable judgment. Let us see. In his address on the 'Principle of Comparison in Physics,' delivered before the German Association of Naturalists and Physicians at Vienna in 1894, that brilliant and versatile mathematical physicist, Ernst Mach, said: "Comparison, as the fundamental condition of communication, is the most powerful inner vital element of science." What is his first illustration of this truth? Again, as in the case of Helmholtz, it is drawn from non-experimental comparative morphology—from comparative anatomy and comparative embryology. "If it is not customary," he continues, "to speak of comparative physics in the same sense that we speak of comparative anatomy, the reason is that in a science of such great experimental activity the attention is turned away too much from *contemplative* element. But, like all other sciences, physics lives and grows by comparison."

It is needless to multiply such statements. Every really rational naturalist must admit that there is but one sane position to adopt, namely, to welcome any and every method by which our knowledge of organic nature may be advanced and unified. No one, I trust, will understand me to advocate the indiscriminate accumulation of facts—for this is not method, but the absence of method. The essence of science is not the accumulation of knowledge, but its organization. Observation and experiment give us our materials, but it is the comparison and correlation of those materials that first build them into the fabric of science. As I regard the matter, it is therefore a reversal of the true standpoint to regard biological classification, in the broadest sense of the term, as no more than a preparation for experiment. Let us, however, admit that our science is entering on a phase in which experimental methods seem destined, and rightly so, to take the leading rank, and that to them we may probably look for the greatest advances that are to be made in years to come. Let us, too, admit that our existing systems of classification, our views

of genealogical relationship, are incomplete, are in many respects still hypothetical and often misleading; that our methods of study have not been sufficiently exact; that a little judicious clipping of the wings of our scientific imagination will render its flight safer, even though it may not soar so high. But let us not depreciate the importance of the comparative study of normal phenomena to which biology already owes so many brilliant triumphs, and which, as we may confidently hope, has still so many future achievements in store. The true aim of the naturalist is to understand the conditions of living forms as they now exist and have existed in the past; but what are these conditions if not the result of an illimitable series of experiments, carried on by nature since life began? Under what other interpretation has the theory of natural selection any meaning? Comparative morphology and physiology but record and coordinate the results of these experiments. The experiments performed in our laboratories but supplement those that have taken place and are always taking place in nature, and their results must be wrought into the same fabric.

One final thought, and I have done. I have said that the key-note of Agassiz's life and work was his love of nature; and in this respect I believe he was typical of the great naturalists of every age. It has of late become the fashion in some quarters to look with a certain condescension on what is styled the 'sentimental side' of natural history, on that keen primary interest in biological phenomena for their own sake, apart from their scientific analysis, that was characteristic of so many of the earlier naturalists. I can but believe that such an attitude shows a lack of insight into the real motives and sources of inspiration of all great observers and discoverers. Every critical analysis of the progress of science leads to a recognition of the vital importance of the imaginative faculty in all research of a high order; and in this regard great masters of creative science, such as Faraday or Darwin, have rightly been placed beside the great masters of creative art. But this faculty is nearly akin to the esthetic sense. Karl Pearson, in his admirable 'Grammar of Science' remarks, "Both works of art and laws of science are the product of the creative imagination, both afford material for the esthetic judgment." Pearson is here referring, it is true, to the sense of beauty and harmony aroused by the discovery and contemplation of natural law. Beyond this, however, we must recognize that there is no more potent spring of scientific research than a lively interest in the facts—in other words, the esthetic satisfaction that lies in the mere observation of natural phenomena. Read the intimate records of the lives of great discoverers in every field of science, and you cannot fail to be struck with this. From this source flows the impulse to analyze by experiment, to correlate by comparison and thus to discover law. The primary impulse of the naturalist is thus given by the love of nature; and I believe that the scientific naturalist should welcome every movement toward the cultivation of general interest in natural history. We may therefore regard it as a happy omen for the future of our science that in every direction we see the signs of increasing interest in field work, in nature-study and in the teaching of natural history in our schools. It would be an evil day for the more advanced and precise study of biology when it came to be regarded as actuated by motives having nothing in common with the love of nature that inspired such men as Darwin, Agassiz, Audubon and Gray; but we need not fear that such a day will come. For my part, I believe that the newer experimental study is better calculated to foster an interest in nature than much of the

minute anatomical and embryological work that has played so great a rôle in the laboratory methods of comparative morphology. These methods were a healthy reaction against the superficial character of much of the earlier work; they form the indispensable basis of all exact and thorough training in biology; but too often in our courses of instruction they have been carried to such a point that the student has lost himself amid anatomical detail of a kind as dry and formal as that of the old-fashioned systematic museum-study. Experimental research is already, I believe, exerting the happiest influence on our methods of teaching by showing how indispensable to a course in comparative morphology is the consideration of physiological phenomena and a study of the living organism.

I cannot better close than with the words that an eminent zoologist—we of this company have not far to seek him—has imagined to be the comment of Aristotle, could he have surveyed some of the aspects of our modern work in biology. "My teaching that the essence of a living being is not what it is made of, or what it does, but why it does it, has been rendered by one of your contemporaries into the statement that life is the continuous adjustment between internal relations and external relations. If this is true, is not the biology which restricts itself to the physical basis and forgets the external world, like your play of 'Hamlet' without the Hamlet? Is not the biological laboratory which leaves out the ocean and the mountains and meadows a monstrous absurdity? Was not the greatest scientific generalization of your times reached independently by two men who were eminent in their familiarity with living things in their homes?" I for one agree with the author of these words that such a comment would be good common sense and therefore good science.

EDMUND B. WILSON.

SCIENTIFIC BOOKS.

Report of the United States Commission of Fish and Fisheries. Part XXV., 1899. By GEORGE M. BOWERS, Commissioner. Washington, Government Printing Office. Pp. clxiii + 397. Plates XXIX + 21.

The contents of this the twenty-fifth report is divided into two portions, of which the first relates to the official and more practical work of the fiscal year, and the second to the special or more scientific work, the preparation of which may have extended over a considerable period.

In speaking of the general condition of the fishery industry, Commissioner Bowers states that the approximate value of the commercial fisheries of the United States in 1899 was $40,000,000, to which the oyster industry contributed about $14,000,000. In comparing the productiveness of the oyster beds of Chesapeake Bay and of Long Island Sound, Commissioner Bowers states that the natural supply of oysters is being exhausted, but that the areas of the sea bottom that are being artificially cultivated are becoming more and more productive. There is sufficient evidence that the increased abundance of cod, in the inshore waters of the New England States, is due to the work of artificial propagation carried on at Gloucester and at Woods Holl. Efforts are being made to rehabilitate the lobster fishery and to devise methods for increasing the number of sturgeon.

Under the direction of Dr. Hugh M. Smith, the Department of Scientific Inquiry has inaugurated or continued several important lines of investigation. The systematic survey of the physical and biological conditions of Lake Erie, begun in 1898 by Professor Reighard, has been continued. Dr. B. W. Evermann has made a biological survey of the waters of the Northwest; Dr. W. C. Kendall has continued his work on the fauna of the lake systems of Maine; and Dr. H. F. Moore has made a study of the physical conditions of Great Salt Lake, and has showed its absolute unfitness for maintaining any form of marine life.

The laboratories at Woods Holl and at Put-in-Bay have been occupied by an enthusiastic corps of investigators, and a building was rented at Beaufort, N. C., to serve as a temporary lab-